sensation. These two incidentals of the psychic life would be monstrously out of place in a trouble-free Eden, in our fool-a-day world they unite to make the cornerstone of human activities.

Using words of broad signification, we may assume that through fear the mind keys the body to instant defensive response for mutual protection. But there was not always a mind. It is a late sublimation of protoplasmic function.

But it is inconceivable that communities of living cells should have failed in the essentials of defensive reaction while waiting the development of a psychological organ. The elaboration of a technique of immunity must have kept pace step by step with the lethal powers of poisonous foes, else sooner or later must the defenseless race have succumbed. It seems probable that this technique accomplishes its purpose of immunity through two different methods.

The immigration of limited numbers of aliens into a growing country adds power to the population when they become assimilated without confusion. What we call "natural immunity" may find its analogue in some such process. But a common historical event has been a mass invasion of one people by another where the issue is not fusion, but conquest of one by the other. In such a case the national spirit of the invaded people is aroused to mobilize their mechanisms of defense, and these may or may not be adequate for victory.

But there is no likeness whatever between the national spirit and the military operations which it called into being. They have different rôles in the warfare. They may combine in a powerful union or, isolated, each may hurry to self-destruction.

The national spirit senses constantly the state of the country in its relations with alien peoples, always with a view of future possible conflict. Defense mechanisms of many kinds, each adjusted to deal with incipient trouble, are trained to go automatically into action. It may be plausibly maintained that this awareness of the country's interests, which is an attitude of the national spirit, finds an analogue in what is termed "allergy" in the phenomena of animal reinfections.

Allergy is the excitement-energy which precipitates the activities of prepared mechanisms of various kinds, here with violent inflammation and high fever, there with the gentle solution and disintegration of foreign germs and poisons without trace. Its whole import and reason for existence is protection of the organism—immunity. The more perfect and powerful that immunity, the less noise of conflict, the less evidence of allergy. But when the strongholds of the defenders are carried by assault—then allergic agony is futile to protect.

These analogues do not in themselves form admissible evidence, but they may broaden that field of suggestion from which the clews to most lines of productive research are picked.

No propaganda for the generation of public opinion can compare in speed and effectiveness with that universal hypersensitiveness to its own products which the growth of tubercle bacilli in a remote corner of the body has for the whole animal organism.

Physicists now soberly tell us that in the last analysis matter and energy are but different phases of the same something. Perhaps disputing biologists may come to see in vitalism and mechanism complementary views of the same subject.

As pain, fear and national spirit may be regarded teleologically as stimuli to conservative defensive reactions on the part of living units or communities, so perhaps we may properly consider allergy to represent a kindred agency which both generates hypersensitiveness to noxious inclusions and stimulates the body cells to form and operate defensive mechanisms against them. According to this view, in the language of social life, allergy is *in* immunity, but not *of* it.

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LEAF DIAGNOSIS AND THE INTERPRETATION OF FERTILIZER REQUIREMENTS OF PLANTS

With an enthusiasm and tenacity that is reminiscent of Pasteur, H. Lagatu and L. Maume, of l'Ecole Nationale d'Agriculture de Montpellier, have made numerous investigations during the past ten years and published *in extenso* on this subject.[1] Recently some of this literature has been widely distributed among the agricultural experiment stations in the United States and elsewhere and has found partial recognition in this country.[2, 3]

The main principles enunciated by the authors are that when one of the major elements—nitrogen, phosphorus or potassium—is omitted from a fertilizer the other two will be absorbed proportionally more, thus leading to unbalanced nutrition, disturbed metabolism and reduced yield. These conclusions are contrary to the widely accepted and amply verified "Law of the Minimum" (Liebig) as applied to nutrition of plants. Experimental data from which

1 *Compt. Rend. Acad. Sci.*, 179: 782, 932, 1924; 180: 1179, 1925; 182: 653, 1926; 184: 229, 1927; 188: 1062, 1929; 190: 389, 1137, 1516, 1930; 191: 579, 1930. *Compt. Rend. Acad. Agr.*, 13: 437, 548, 1927, and 14: 762, 1928. "Le diagnostic foliaire de la pomme de terre," *Ann. Sci. Agron.*, 47: 5: 596, 1930.

2 Science, 70: 382, 1929.

3 Science, 72: 425, 1930.

these deductions are drawn have been secured by the above authors, largely from the grape, *Vitis vinifera,* and more recently from the potato, *Solanum tuberosum,* the plants having been subjected to various régimes of nutrition. These records are primarily in the form of chemical analyses of leaves for N, K_2O, P_2O_5 and CaO.

In the extensive literature on plant nutrition, one finds frequent references to the general appearance of the plant, particularly its foliage, as being symptomatic of the deficiency of certain major or minor nutrient elements. In fact, Russell[4] gives a comprehensive list, which shows the apparent relationships between leaf appearance and lack of particular nutrient elements, but cautions that "if they (symptoms) are to be used in any but a general way, they should be studied by setting up the appropriate vegetative experiments" in order to verify the relationship between a particular symptom and its probable antecedent cause. Few[5] investigators, however, have gone so far as to base on chemical analysis of leaves either a diagnosis of the state of nutrition or to suggest or prescribe a program of soil fertilization. To the writers' knowledge leaf diagnosis (*diagnostic foliaire*), as conducted by Lagatu and Maume, is the first serious attempt in this direction. Is this method and technique really applicable in studies of nutrient requirements of plants, especially perennials? A partial confirmation comes from Wallace,[6] who found that the omission of potassium from a "complete fertilizer" applied to soil resulted in an increased absorption by *Ribes grossularia* of nitrogen and phosphorus. But when either nitrogen or phosphorus was omitted from the fertilizer then there was a decreased absorption of the other two elements.

The present writers had an excellent opportunity to test the principles proclaimed by Lagatu and Maume during the course of study of the physiology and metabolism of the apple (*Pyrus malus*). Dwarf trees were grown in pure quartz sand cultures and were supplied with nutrient solutions containing a constant and optimal concentration of phosphorus, nitrogen and potassium. For two groups of plants, however, the concentration of N and K in the nutrient solution varied respectively from 0 to 323 and 368 parts per million (ppm). At appropriate periods leaves and twigs were subjected to a chemical analysis for total N and K.

It is quite evident that the reduction or total omission of either nitrogen or potassium in no instance

ANALYSIS OF LEAVES OF PYRUS MALUS

Nutrition		Chemical composition	
K and P	N, ppm	Total N, per cent.	Total K, per cent.
Constant	0	1.52	0.89
Constant	110	1.45	1.10
Constant	323	1.73	1.49
P and N	K		
Constant	0	1.38	0.94
Constant	126	1.45	1.10
Constant	368	1.50	3.04

resulted in an increased potassium, respectively nitrogen content of leaves. Quite the contrary; with a decrease of nitrogen in the nutrient medium there was a decrease in potassium concentration in the leaves and with a decrease in potassium there was a decrease in nitrogen. Moreover, the relative concentration of N and K in twigs was exactly of the same order, but, of course, of a different magnitude. (More detailed data on the chemical composition of leaves and twigs of these trees will be published elsewhere.)

These experiments furnish a concrete evidence and corroborate the negative results secured by Wallace and by Thomas[3] with the additional proof that, contrary to Wallace, the omission of potassium did not increase but evidently decreased the absorption of nitrogen. Consequently the principles proposed by Lagatu and Maume have not been verified, and Liebig's Law of the Minimum, as amplified by Mitscherlich[7] and others, must be considered as our best guide in the nutrition of plants.

But, disconsidering their codified generalization, why have Lagatu and Maume obtained seemingly consistent results with the grape? A part of this discrepancy between our and their results most probably is due to the fact that each species (in these instances *Vitis vs. Pyrus*) absorbs soil nutrients in a "physiologically balanced" proportion. Hence we have to deal here with selective or differential absorption, ionic antagonism and related phenomena (Loeb, Osterhout) as is suggested by Thomas.[3] Thus one is referred to the soil, more properly to the soil nutrient solution, and to the specific régime of fertilization of their plants as the primary causal source of Lagatu and Maume's results and Mitscherlich's[7] method of interpretation should be applied.

It seems to the writers that other equally, if not more, potent factors may be advanced to account for and to assist in interpretation of the data obtained by Lagatu and Maume. The concentration of certain soil nutrients in particular organs by no means re-

[4] E. J. Russell, "Soil Condition and Plant Growth," p. 102, 1927.

[5] Th. Remy-Brown, *Mittl. Deutsch. Landw. Gesell.*, 28: 416, 1913.

[6] T. Wallace, *Jour. Pomol. and Hort. Sci.*, 7: 1–2, 130, 1928.

[7] A. Mitscherlich, "Die Bestimmung des Düngerbedürfnisses des Bodens," 1925.

flects directly and definitely the rate or the proportionality of absorption of particular elements by a plant. Indirectly it may, and often does, indicate likewise the utilization of these elements by other organs, especially those of a higher metabolic rate and hence of a more rapid development. Due to a close physiological correlation of the various organs of a plant there is a continuous removal and diverting in unequal proportions of certain elements from particular organs, like leaves, and their reutilization for the development of other parts—fruits, shoots and roots. This is particularly true of woody perennials, like *Vitis*. It has been demonstrated by one of the writers[8] and by others[9, 10] that every one of the three elements—nitrogen, phosphorus and potassium —is removed from the lower leaves of the tomato plant whenever a shortage of a particular element for the vegetative extension or fruit development occurs. Yet Lagatu and Maume base all their interpretations solely on the analysis of basal leaves of fruiting canes of *Vitis* and of tuber-bearing plants of *Solanum*. It is not difficult to see how the concentration of any two of the three major elements of soil nutrients may increase in the leaves when one is *in minimo* for the development of other metabolically more active organs. This increase, therefore, is due to two major factors (not one): An unbalanced fertilization of the plant as a whole and an unbalanced nutrition of particular organs. In either instance (below and above ground, the "intake" and "outgo," absorption and utilization) the law of the minimum seems to hold true.

The detailed mechanism and interpretation of the metabolism and physiology of organic correlation may be analyzed on the basis of "metabolic gradients," as suggested by Child.[11]

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THE MECHANISM OF CROSSING-OVER

DARLINGTON recently published in SCIENCE[1] (73: 561–562) a criticism of my work[2] which might give the reader the impression that Darlington was not given credit for previous suggestions concerning the mechanism of crossing-over. In 1929 Darlington[3] did suggest that crossing-over is due to a reduction of chiasmata by breaking. In the three sentences which refer to this theory (pp. 50, 51 and 52) he does not explain how chromatids might break and recombine nor does he present any evidence to show that he had any conception of the significance of his suggestion. However, his suggestion was acknowledged (Sax, 1930, p. 209) as follows: "It seems very probable, however, that crossing-over between homologous chromatids is associated with the reduction in the number of chiasmas between diplotene and diakinesis, as Darlington (1929) has suggested."

In a paper which was published shortly before mine went to press, Darlington[4] does not mention his earlier suggestion that crossing-over is caused by breaks in the chiasmata, but expresses the view that cross-overs determine chiasma formation. The cytological evidence for this view is supported by a few diagrams and text figures which by no means can be considered as a "cytological demonstration of genetic crossing-over."

The fact that Darlington discarded his earlier "suggestion" on crossing-over in no way discredits or weakens my theory. Darlington has made so many assumptions concerning chromosome pairing, and has changed his mind so frequently that one would necessarily have to cite one of his numerous theories in any discussion of crossing-over.

Darlington also states that I have used diagrams and terminology borrowed from his 1929 paper. The only term used which might be credited to Darlington is "terminalisation." No figures were borrowed from him, although my figures 9 and 10 are based on his work. For this phase of my interpretation of crossing-over Darlington is given credit as follows: "The behavior of the chromosomes in triploid Hyacinths described by Darlington (1929) seems to offer an explanation of triploid crossing-over" (p. 214). The fact that Darlington did not fully appreciate the genetic significance of his cytological results in no way discredits his ability as a technician.

According to Darlington, my "genetical remarks might be taken to favor either hypothesis—for there is no decisive evidence between them." This statement is of more than doubtful validity. In none of Darlington's papers is there any explanation of the cause of breaks in chromatids, why they unite in new associations, why the two homologous chromatids almost always cross over at the same loci, of how gene duplication or deficiency could occur, or why one cross-over interferes with another. In his most re-

8 A. E. Murneek, *Plant Physiol.*, 1: 3–56, 1926.
9 J. H. MacGillivray, *Jour. Agr. Res.*, 34: 2: 97, 1927.
10 G. Janssen and R. P. Bartholomew, *Jour. Agr. Res.*, 38: 8: 447, 1929.
11 C. M. Child, "Individuality in Organisms" and "Senescence and Rejuvenescence," Univ. of Chicago Press, 1915. Also *Plant Physiol.*, 1: 1–3, 1926.

1 C. D. Darlington, "The Mechanism of Crossing-over," SCIENCE, 73: 561–562, 1931.
2 Karl Sax, "Chromosome Structure and the Mechanism of Crossing-over," *Jour. Arnold Arb.*, 11: 193–220, 1930.
3 C. D. Darlington, "Meiosis in Polyploids. II. Aneuploid Hyacinths," *Jour. Gen.*, 21– 17–56, 1929.
4 C. D. Darlington, "A Cytological Demonstration of 'Genetic' Crossing-over," *Proc. Roy. Soc.*, B. 107: 50–59, 1930.